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TOLERANCES AND THEIR EFFECT ON PHYSICAL MEASUREMENTS

by SIR CHARLES DARWIN, K.B.E., M.A., F.R.S. *(Director of the National Physical Laboratory)*

(Address delivered on May 9th, 1944)

SUMMARY.

The tolerances that may be admitted in manufacturing processes have always been an important matter in works practice, but they have tended in the past not to be considered as a subject for deep study, but rather as a nuisance which would not arise in a perfect world. Recent developments in theoretical physics have very much altered this view, and have shown that the subject of errors and tolerances is not only interesting in itself, but is important in the foundations of physics.

A series of examples is given showing some interesting points in considering tolerances. It starts with the large numbers concerned in accident rates, and then takes a simple manufacturing process. Next, examples from acoustics and optics are considered, and then the "Uncertainty Principle" of the quantum theory, which lies at the basis of all physical measurement. Finally, it is shown that even pure mathematics is essentially based on the idea of tolerances.

The makers of almost any kind of mass-produced article are very familiar with all the troubles that arise in working to assigned tolerances in their manufacturing processes. I expect for the most part they regard it is a nuisance having to do so, and that in a properly regulated world it is the sort of thing that would be left out. After all, we none of us like dwelling on our own imperfections, so why, when we have said that we are going to make something an inch long, should we have to waste time over those little peccadillos which make it a few ten-thousandths wrong. The physicist has been even worse in this respect, because he was always formulating exact laws and then trying to verify them experimentally. Of course, he had errors in his experiments, but they were mostly just a nuisance, and he was usually not interested in them in themselves, but only as an impediment to the realisation of what he thought of as an absolute truth. For his purposes he was often quite right in this, but it led to a general outlook from which errors and tolerances were excluded.

Teachers were still worse in this respect. Just as our theological teachers will try to prescribe what will constitute perfect virtue, and will not devote much time to, shall we say, calibrating the various degrees of vice that are inherent in all human conduct, so the teacher in an engineering school has in the past tended to describe only perfection and not to give much attention to the faults which in fact always occur in manufacture. If I may commit what almost amounts to a pun, I would say that teaching has tended to be entirely intolerant. I have for a good many years been interested in this neglect in our outlook, and I was frankly shocked only three or four years ago when I put it to the proof. I happened to be visiting a member of the staff of one of our leading engineering schools, and I asked him what the students were taught about tolerances. He looked a bit blank, and said, "I don't know; let's have those two boys back who have just gone out and ask them." So the students (who were second-year scholars) were called back, and I asked them the question of what the word "tolerance" meant. One simply did not know. The other gave the correct answer, but on cross-examination I found he was really cheating, because he happened to be the son of a prominent member of your society and had learnt about it at home.

The main purpose of my lecture is to emphasise the point that tolerances have now become a respectable part not only of engineering practice, but also of fundamental science, and that a study of their principles can be a really interesting subject. You will naturally at once think of the great developments in the field of Quality Control of production that have been taking place in the last

few years, a subject in which the manufacturers of electric lamps have taken the lead. I am not going to discuss this aspect; to do so would involve going rather deeply into technical mathematics. I would only say that the point of it really is that once you recognise that there are to be errors you find that they do have very prevalently a certain distribution, so that large ones become rarer than small according to a rather well defined rule, and that you can exploit this rule so as to get a great deal more out of your measurements than you might expect to at first sight. The process is still in its infancy, and I have little doubt that improved methods of sampling will lead to considerable economies in the application. But it falls outside my subject to-day, because I intend to show that a great many interesting things can emerge merely from the idea that there will be errors, either of manufacture or of experiment.

My first example is with regard to whole numbers. Here is a case where you might think there could be no error. There are either 100 or 101 men in this room listening to me, and there cannot be both. But it is very likely that, when the number is large, someone will have come late and someone else will have to go early to catch a train, so what is the answer then? One interesting example of the tolerance of large numbers is the rate of accidents. They used to publish the number of people killed per year in motoring accidents. It used to be at a rate of about 500 a month. What fluctuation would be expected? The law of chance answers immediately that it will be about the square root of this, that is to say, 23, and that anything like a fluctuation of 50 either way is distinctly unlikely to happen more than about once in two years. Notice that from this we can say with considerable confidence what is the safest month to live through; it is February, because that is three days short in 31 that is, 10 per cent., so that there should on the average be 50 fewer killed, and this defect is very unlikely to be beaten by the chance fluctuation. If you examine anything which depends on whole numbers, you will nearly always find that if they are larger than two figures they are not exact, in that there is nothing essential about their exactness.

My next example is interesting to me personally because it happened to be the thing which suggested to me about 12 years ago what an interesting subject in general this was —before that I had only thought about it in connection with ultimate physical theory. I happened to be visiting an engineering works where they were making for Woolwich a machine which was to weigh bullets. The bullets were to be used for the testing of armour and a rather high accuracy was asked for. The machine was, I imagine, much like those used in the Mint for weighing coins, but as it had to handle a great many bullets speed of operation was important. It worked like this: The bullets were pushed in a row along a groove. At one place each in turn came on to a balance with the proper counterpoise and a lever freed the balance. If the bullet was right the balance would stay level, and the next push would send it straight on. But if the bullet was too heavy or too light the scale would fall or rise and the push would throw it out—I cannot remember the exact mechanism. The question then is, how quick can the balance be made to work? Obviously, if a very close tolerance is needed, a bullet which is just to be rejected will only be a little wrong, and so will only tip the scale very sluggishly. I took a tolerance of one part in a thousand. It is a very straightforward problem in elementary mechanics, but I shall not inflict it on you. The point is that with the given value of gravity, and with the inertia which the system must have because of the bullet itself and the counterpoise, one can easily see how long it will take to rise to a height of the diameter of the bullet. If one attempted to discriminate earlier than this time the guide for the correct bullets would get in the way of the rejected ones. The answer came out that it really did not matter what one did, one could not weigh the bullets faster than about

one a second. I ought to say that here, and in most of my subsequent argument, I am going to practise a type of arithmetic which has attractive features for most of us, because I do not pay any attention to mere things like 100 per cent. changes. When, therefore, I say that you can't do more than one a second, what I mean is that you might do two or you might possibly only be able to do one every two seconds, but that you certainly could not do ten a second, and certainly could easily do one in ten seconds.

The moral of this example is that it takes time to do a weighing, and that the more precise it is to be the longer it will take. It is easy of course to imagine machinery by which, at the price of a good deal of complication, I could speed up the process. For example, I might have a long boom projecting from the balance midway between two contacts which were rather close together. Then a much smaller motion of the balance than I took before (which was the diameter of the bullet) would show which way the scale was going, and I could imagine a grab coming out and knocking away the rejects. I do not say this would be a practical machine to make; indeed, it would certainly be simpler to make ten of the simple machines and work each of them more slowly, but the point is that we can imagine ways of speeding up the business beyond our first estimate. I mention this because later on I am going to come to an absolute limit of time that is essential for weighing anything to any assigned accuracy.

My next example is from the theory of sound. Briefly, the question is this. A man sings a note; how long is it before I can say whether he was in tune? To define the experiment we might suppose that a tuning-fork has been struck, and that he is trying to sing the same note. A musical note is a harmonic vibration, so that he is sending out a train of waves of a certain frequency, and so is the fork. If he is out of tune his frequency will be a little wrong, which means that whereas at first his maximum of air pressure was in step with that of the fork, after a certain number of cycles he will have got out of step. When a piano tuner wants to get two notes in tune together he listens to the beats between them, and gauges the accuracy by the seldomness of the beats. He certainly needs to hear two or three beats before he can say if they are tuned, but we will be generous and imagine that a single beat would suffice. Thus we can only say the man is out of tune with the fork if there has been one beat between them; that is to say, if there is time for the man's sound wave to put in one more oscillation than the fork has done. Let us come to numbers. The man is to try to hit the C below middle C, and we will not criticise him if he is nearer to it than a semitone. This C has a frequency of about 120 cycles a second, and a semitone is about twentieth in frequency. So we will pass anything between 114 and 126 as our tolerance. The difference of six cycles means that after a sixth of a second the man will have completed one cycle more or less than the fork; that is to say that there will have been the one beat that we are looking for. So the answer is that the man can sing for a sixth of a second before we can find him out. A woman has a much harder time. Say she is singing the upper C. This has four times the frequency, and therefore a semitone, again a twentieth, is now 24 cycles different. She therefore gives herself away in a twenty-fourth of a second. It is very easy to verify what I have been saying. If you try to play a shake of a semitone on the piano, you will find that if you do it in the rather deep bass the whole business seems to become meaningless. This is a matter of pure physics (though no doubt the character of the human ear reinforces the trouble), in that the notes struck are really indefinite, because each note is not given time to define itself.

Next, I am going to take an example from optics. It is the typical example of the idea of "resolving power," which was so well worked

out by Rayleigh about 70 years ago. The principle is most important in connection with lenses, but there is a simpler case which gives the whole gist of it. When we want to use a mirror galvanometer for very sensitive measurements we set the source of light and the observing telescope both a long way off, so that a very small deflection of the galvanometer needle gives a large motion of the image. At first sight it would be tempting to think that we could increase the sensitivity indefinitely by increasing the distances, but this is not so. For consider how the image is formed. We will suppose an appropriate lens is put somewhere in the path to focus the filament on a scale. The bright spot on the screen is at the place where all the rays from the lamp have exactly the same number of wavelengths to the screen for every point of the mirror. Now suppose the mirror turns a little, but think of the same place on the screen. We shall expect to detect the deflection when this point first goes dark. When will that be? The answer is when the rays that go from the light via one edge of the mirror differ by about a wavelength from those that go via the other. The intermediate parts of the mirror will then give light in phases in between, and the whole lot will cancel out. Thus the smallest turning one can detect is when one edge of the mirror has advanced a wavelength compared to the other. It may happen to be convenient to do this by setting up the screen a long way off, but it is just as good to use a more powerful telescope to look at it from near by, or even, as an extreme case, to take a microscope and watch the mirror edgewise itself, for a good microscope can detect just one wavelength of light. So the result that emerges is that it is the breadth of the mirror that matters and not the distance at which the screen is set up.

A wavelength of light is pretty short, but the principle that we cannot see detail smaller than a wavelength does not satisfy us, and we want to know whether the finer details cannot be observed in some other way. One obvious trick is to use a shorter wavelength, both by oil immersion and by using ultra violet light. The electron microscope does the same sort of thing to a higher degree, and because it has attracted attention recently I might digress a little to say something about it, even though it will have no very direct bearing on my main subject.

Both types of microscope work on much the same principle. Let us suppose we want to study the shape of a small black dot. In either case we first must have a condenser to throw rays of light, or of electrons, on to the field of view. To get good resolution calls for a strong condenser, that is to say, rays coming in on to it over very wide angles. The ones that fall on the dot are stopped, but the rest go through, and the microscope lens focuses them on to a photographic plate—or possibly the focal plane may be viewed with an eye-piece. The lenses that bend the electrons are either electric or magnetic fields, without of course the sharp surfaces of glass lenses, but they work in much the same way and all the elementary formulae of geometrical optics apply to them, including the ordinary relationships of distances and focal lengths to magnifications. As to the resolution, we have seen it depends for light on the wavelength, and we must ask what corresponds for the electron. It was discovered only about 20 years ago that this was a question that could be asked at all, but once recognised the answer could be given. I fear it would take too long to discuss this properly, though in fact I shall presently be talking on the same subject from a different point of view, and the connection will not be very apparent. Here I can only say that the resolution is proportional to the voltage of the electrons, and for the voltages used, such as 60,000, it is about 10^{-8} cm. It looks as if we should be able to see single atoms, because that is known to be about their size. Unfortunately, there is a catch in it. The resolution of a microscope depends on the angles through which the rays are bent, and for the highest class of microscope

the extreme angle should be nearly a right angle. This calls for a very elaborate lens system in order to correct the various aberrations. No one has yet succeeded in correcting any of the aberrations of an electron lens at all (and theory suggests doubt as to whether it will ever be possible to do so to any considerable extent), so that in fact the rays in the electron microscope can only be deflected less than a degree of arc, or the image becomes spoiled. The consequence of this is that a great deal of the resolving power is sacrificed, and the most hitherto gained over the optic microscope has been a factor of about 50 in magnification, instead of about 2,000 as simple consideration might at first sight have suggested. One might hope to improve things by putting up the voltage, so as still further to shorten the wavelength, but another trouble then comes in, because the electrons pass so easily through the object that the image becomes too faint to see.

When we want to get down to seeing smaller sizes we know we need shorter wavelengths, and this suggests using X-rays. This has already been done long ago indirectly, but no one has yet made an X-ray lens; however, X-rays are exactly the same thing as light, and so we may legitimately imagine we could do this. This would take us to about 10^{-9} cm. The gamma rays would take us a good bit further, and after that we might imagine we could use the cosmic rays—perhaps with a little hesitation, because they are still rather mysterious. In this way we might suppose we could see things of the size of about 10^{-13} cm., and it might seem natural to ask, What next? But the whole subject received a twist about 20 years ago in a different direction, and this I must now describe.

You know that during the first 25 years of this century the quantum theory was a disturber of the peace in the ideas we all had about ultimate mechanics. It was so obviously right and yet it so hopelessly contradicted all the laboriously compiled, and also obviously right, ideas of the older physics. The conflict between the two was increased about 1922 by an American physicist, Arthur Compton, who first propounded a theory and then verified it experimentally. His idea was this: He asked the question, What happens when a light wave passes over a free electron? The old answer would have been, unhesitatingly, that it shakes it to and fro a little, and in consequence the electron scatters a little of the light steadily all round. Not so, said Compton. According to the quantum theory, though light has wave properties, it also has qualities like a particle, and my guess, from general analogy with other parts of the quantum theory, is that this is a case where it will be like a particle. Therefore when the light passes the atom there will be a certain chance that they will behave like a pair of of billiard balls, so that the electron will suddenly be thrown off in one direction and the light will bounce off into another, as in billiards. To get a good large effect you must use a heavy light-particle. Ordinary light is not heavy enough, but X-rays will be. So the experiment had to be done not with light but with X-rays, and with these he brilliantly verified it. There are a great many interesting things about this effect, but I shall not go into them because the only one that I need is that when light passes over an electron, if the electron scatters the light, then the light also scatters the electron; that is to say, it gives it a flip in some direction. This flip is larger the harder the X-rays; that is to say, the shorter their wavelength.

In the year 1926 the solution of the main contradictions between the old theories and the quantum theory was found. A number of people were concerned, but chief among them was a young German physicist, Heisenberg. At first the solution was in a rather abstract mathematical form, but it was not long before Heisenberg himself discovered a way of understanding it physically. He asked the question, What happens if you really start trying to decide where an electron is? You might go about it by

making a microscope, but to get satisfactory precision you would take an X-ray microscope because of the short wavelength—true, we don't know how to make one yet, but we can be reasonably satisfied that it is a legitimate thing to imagine. With this you look to see where the electron is; that is to say, you shine X-rays on the electron, which scatters the light and this scattered light you then focus in the microscope and look at the image. But now comes the catch. You won't see the electron unless it has scattered the light, but if it does scatter it then it comes under the Compton effect and is flipped away. So you do not see where the electron *is*, but only where it *was*. To weaken this trouble you might go back to ordinary light, for which the Compton effect is very small, but in doing so you have given up the idea of fixing its position so accurately because you are using a longer wavelength for your microscope, and in a microscope you can never see a position with accuracy better than the wavelength. This is the Uncertainty Principle, that it is not possible to fix both position and velocity of a body simultaneously with unlimited accuracy. There is no limit to the tolerance you may demand for either, but if you make exacting demands on one that automatically makes you relax your demands on the other. Notice the new twist the subject has got. There is nothing wrong with the conception that the electron has a definite position and velocity, but it is not a useful idea because you can never find them both out at the same time. In the very act of trying to do so you spoil things and ensure that they will be different from what they were before. When the theory is worked out in detail the answer can be expressed very easily in terms of tolerances. Measure the position in centimetres and the velocity in centimetres a second. Then the most accurate tolerances that can be ever obtained obey the rule that the tolerance of the electron's position $\triangle x$ multiplied by the tolerance of its velocity $\triangle v$ obey the rule.

$$\triangle x \times \triangle v = 7 \text{cm}^2/\text{sec}.$$

I ought to mention that a similar rule holds for other bodies, but the number on the right is inversely proportional to the mass, so that since the electron is the lightest body existing, it is the one for which the effect is most marked. Since the mass of an electron is about 10^{-27} gramme you can see that the effect for ordinary bodies is not of a magnitude to cause serious trouble.

The Uncertainty Principle is quite fundamental in modern physics. Its effects are inescapable, so that we can see how the idea of tolerance has got to be accepted as one of the basic ideas, and can no longer be fobbed off by saying that with deeper knowledge we could get rid of it. It is the Uncertainty Principle that has finally made tolerance respectable, as I told you earlier it has now become.

There are other ways in which it can be expressed, which from the theoretical point of view are much more powerful, but I shall not go into them. I shall only take one further rather different example of it, because it will connect up with one of my earlier examples, that of weighing the bullets. In Heisenberg's example fixing the position spoiled the velocity, which we express by saying that position and velocity are complementary. In the same way mechanical principles show that energy and time are complementary. Now the principle of relativity shows that energy and mass are the same thing, and therefore the Uncertainty Principle asserts that it takes a certain time to determine a mass with a given precision, and that the smaller tolerance we demand in the mass the longer we must take in doing the weighing. I shall not attempt to design and explain an experiment to show how this comes about, nor shall I go through the calculations; but I will give you the result only. Suppose our bullet weighed 10 grammes, and that as before we

want it to a tolerance of one in a thousand, that is to the nearest 1/100 gramme. Then you cannot hope to do this in a time of less than 7×10^{-45} sec. There is evidently a possibility for improvement in our present methods, which gave a time of 1 sec.

Are there any further limitations to tolerances in nature besides the Uncertainty Principle? We do not really know at all, but there have been rather plausible conjectures that there are; that, for instance, there is no meaning in talking of a distance shorter than 10^{-13} cm., which is about the size of the nucleus of the atom. I shall not attempt to discuss these rather vague ideas, but at the risk of an anti-climax I shall turn over to quite a different field, where the idea of even finer tolerances is important.

This is the field of pure mathematics. You will all have heard of the celebrated problem of Achilles and the tortoise. The answer that Achilles would really catch up the tortoise was just as obvious to the Greeks who propounded the problem as it is to us, but that is not the point. The point is that when you start trying to get down to pure logic there are contradictions involved in the idea of a line being made up of a row of points, and the argument can be presented in such a way that it is very hard to see what is wrong with it. That it was hard may be seen by the fact that it took over 2,000 years to find the solution. This solution was the act of a number of mathematicians about 100 years ago. Their work, being of a logical kind, had to be worded with extreme care, and if you read the proofs they give of some of their important theorems you are reminded of the way in which a conveyancing lawyer draws up a rather complicated deed, putting into it a lot of things you never thought of. In the course of these proofs great use is made of a particular letter, the Greek letter ϵ; I don't know why it is always this letter that is used, as there is no particular reason why it should be, but the subject is sometimes irreverently called epsilonology on the strength of it. Now the whole thing would have been really fairly simple if mathematicians had been accustomed 100 years ago to the idea of tolerance, because ϵ is nothing but the tolerance.

I am going to give you an example. What do we mean by $\sqrt{2}$? There is no number which, multiplied by itself, gives 2. This troubled the Greek mathematicians a great deal, and indeed they called such numbers irrationals; that is to say, unreasonable numbers. In modern times it became clear that though there was no such number, one could pretend there was, and play with it in arithmetic as if it were a genuine number; but still it was not a rational or reasonable number, and no one could answer the difficulties of the Greeks. The correct answer was given 100 years ago in this manner. *You* say there is no such number. *I* say to you that if you will assign any tolerance you like I can give you a number which will have a square nearer to 2 than your tolerance. I don't care how close a tolerance you fix, I can beat it. The way is obvious. If you say a hundredth I give you the square root to two places of decimals; if you say a millionth I give it to six places, and so on. The arguments about irrationals are sound, not because irrationals are numbers in the ordinary sense, but because whatever tolerances are assigned numbers can be found which are closer to the result. I am not going to say that this description would suit the lawyer-like habits of the pure mathematician, but it is not a bad definition of his subject to say that pure mathematics is the branch of physics which is true for *any* and *every* tolerance.

I have now traversed the field of tolerances from large numbers, through objects of ordinary size, then through objects so small that we need a powerful microscope to see them, then to the ultimate physcial realities, and now down to anything of completely unlimited size. There is no further to go, and I can only hope I have convinced you that the subject is an interesting and important one.

THE EFFECTIVENESS OF LIGHTING: ITS NUMERICAL ASSESSMENT BY METHODS BASED ON BLINKING RATES

It will be recalled that the paper on the above subject read by Mrs. Sheila J. Macpherson on February 9, 1943 (Trans. Ilum. Eng. Soc., London, March, 1943, p.48), has since been reprinted, by request, in *Illuminating Engineering*, the Journal of the Illuminating Engineering Society in the United States.

We have received from Dr. Matthew Luckiesh (Director of the Lighting Research Laboratory of the General Electric Company, U.S.A.) a copy of his contribution to *Illuminating Engineering* commenting on this paper, which is reproduced below, together with Mrs. Macpherson's remarks in reply.

Dr. M. LUCKIESH:—

In "Illuminating Engineering," November, 1943, there is reprinted from The Transactions of the I.E.S. of Great Britain a brief article by Sheila J. Macpherson, in which she attempts to appraise the blink-rate as a criterion of "ease of doing visual tasks." The data which she presents are too fragmentary to be of much import in criticising the extensive work of myself and Frank K. Moss, which continued over a decade, with many subjects in two score extensive researches,(1) with various controllable factors individually used as the controlled variable. Our results with the rate of involuntary blinking correlate very well with the results obtained by other criteria and are consistent among themselves. In so far as Mrs. Macpherson's work bears upon ours, she obtains the same qualitative results. We have never given any importance to our results quantitatively, excepting as a measure of their reliability. We have been satisfied to state that by the blink-rate criterion one condition provides easier seeing than another condition.

The main point that seems to have escaped Mrs. Macpherson is that all our work for a decade—with one exception—involved *reading*. We have never even intimated that the blink-rate could necessarily be used to compare reading with any other seeing task. In fact, it is well known that the same individual will blink at different rates when reading, writing, thinking, sewing, or what not under the same seeing conditions. We recognised this in the years when we were considering the possibilities of the rate of involuntary blinking before we began a decade of extensive research with the blink-rate as a criterion of ease of seeing.

In the second paragraph, Mrs. Macpherson apparently quotes from page 224 of one of our earlier books(2). The word *specific* refers to any one of the five tests summarised in Table 33. Four of these five tests involved reading good typography under different seeing conditions which varied "ease of seeing." Since the subjects read books of their own choosing "the task of reading for an hour was not a difficult one when appraised introspectively."

It is not surprising that Mrs. Macpherson obtained different quantitative results for the increase in the blink-rate while reading under glare conditions. She used a different group of subjects, and it is very likely that the seeing conditions—surroundings, reading matter, glare-source, etc.—were quite different from ours. The important fact is that her results substantiated ours in showing an increase in the blink-rate while performing a difficult visual task.

The measurement of the change in rate of blinking is not necessarily a complete or perfect measure of eye-strain and should be applied with the greatest caution in tasks such as some of those described by Mrs. Macpherson. For example, one of her tasks involved counting a row of identical dots. If the observer knows that blinking will cause him to lose count, he may forcibly restrain himself from blinking until such time as it will not affect his counting. Consequently, his blink-rate might be expected to be lower than when the task is a much simpler

one, such as reading, where a blink will not cause him to lose his place.

We fully recognise the "low initial blink-rates which may be caused by

(1) over-anxiety and concentration on a task and;

(2) tasks whose performance would be impaired by rapid blinking."

In describing the results of one test in which the subject had to look back and forth from one field to another of the same brightness and to distinguish a letter which appeared in each field, we stated(2) that "It is also conceivable that the observed decrease in the rate of blinking is due to the fact that excessive blinking interferes with the performance of this rapid and exacting visual task. In any event, the criterion of eyelid response is applicable for appraising the relative difficulty of various visual tasks only after allowance is made for any change in the base rate of blinking due to such reflex effects."

It cannot be over-emphasised that the actual performance of a task of ordinary reading in a completely normal manner is just as much a part of our blink-rate technique as the blinks themselves. Furthermore, the blinks were always counted visually by another person. We would not thinking of risking the disturbance of a mechanical counter operated by the eyelids. We did not even use our very highly sensitive device with which, by attaching electrodes to the forehead and temporal regions, we obtain electromyograms of the action currents in the eye-muscles. In other words, a life-long experience in psychophysiological research has revealed to us many pitfalls which may be overlooked by the uninitiated.

The fact that different persons have greatly different blink-rates, while reading the same material under the same conditions, causes no difficulty. The same group of subjects is used for studying a given controlled variable. We are interested only in a decrease or increase in the blink-rate and not in the magnitude of decrease or increase excepting in so far as it indicates reliability of the results.

Most of Mrs. Macpherson's comments in her article have no bearing upon our work, because we dealt only with the task of reading. We chose reading because it is a controllable, describable, common task. Our subjects were always educated adults, generally between twenty-five and thirty years of age.

Mrs. Macpherson's work is interesting and I hope she continues it. In so far as it bears upon our work at all it is gratifying to note that it is in general agreement. In fact, I know of no published results that can be properly compared with ours, which are in disagreementw ith our results. Unfortunately, little or no work has been done which involves exactly our technique. In a discussion, Logan (Transactions of the Illuminating Engineering Society, 36, 1941, 1057), states that Dr. Thorndike did not obtain the results we did. The fact is that Thorndike performed a few casual tests which, as he told me, were inadequate for publication. In the same discussion, Logan stated that results by McNally disagreed with ours. I was familiar with McNally's work, which was with young school-children, while he was planning it. It was neither extensive nor comparable with ours in technique. In the same discussion Logan mentioned work by Dr. Hardy. If this has been published it has escaped my attention.

From a decade of researches by myself and my esteemed colleague, Frank K. Moss, we have arrived at the conclusion that the rate of involuntary blinking *while reading* is a very sensitive criterion of ease of seeing, and that the results are significantly related to tenseness and other results of reading and other critical seeing. This is, indeed, a complex field of research, and extensive researches must be performed in the most careful manner by qualified individuals with the best of understanding and control of the factors involved. Our work is the only extensive series of researches as yet performed. I am convinced that the rate of involuntary blinking while performing the *same* task under different conditions is by far the most sensitive, significant, and practicable criterion of ease of seeing which has been extensively studied as yet. One of its chief assets is that the blink is involuntary and outside the sphere of

conscious control while experienced and non-curious subjects are performing such a task as reading.

I refer those interested in all the theory and philosophy behind the involuntary blink-rate, the facts yielded by it, our manner of apply it, and the limitations which we have recognised, to the references given. Space does not permit discussing these aspects in detail here.

References:

(1) Summarised in "Reading as a Visual Task." Matthew Luckiesh and Frank K. Moss. (D. Van Nostrand Company, New York, 1942.)

(2) "The Science of Seeing." Matthew Luckiesh and Frank K. Moss. (D. Van Nostrand Company, New York, 1937.)

Mrs. S. J. MACPHERSON:—

In Dr. Luckiesh's book "Science of Seeing" (p. 223) he implies that blink-rate-increase as a criterion of "ease of seeing" might have a wider application than to reading only. He states: "It has been shown that the performance of critical visual tasks produces certain reflex effects which are considered to be of central origin and which may be correlated with the expenditure of human energy in seeing. . . . Hence it may be supposed that eyelid movements, since they are supposed to be centrally controlled, may also be caused by "reverberations" in the central nervous system, and hence correlated with factors pertaining to the expenditure of human energy in seeing."

It is not made clear that he only intended the "critical visual tasks" to apply to reading. It was thought worth while, therefore, to try this as a criterion of "ease of seeing" in a visual task of current war importance in which eye-strain has been complained of. However, my measurements of blink-rate in this and other critical visual tasks, apart from reading, had shown that increases in blink-rate were unreliable as an indication of difficult or inefficient seeing conditions. The question before the Illuminating Engineering Society was also a wider question than that of reading; it was the assessment of effectiveness of lighting. I do not think it could be contended that reading type of a certain size in a light intended for some totally different work could necessarily be used as an indicator of its suitability for the task in hand.

The field of application of the blink-rate-increase test therefore becomes a rather narrow one. Even within this field our experience has been of great irregularity. Dr. Luckiesh himself says ("Reading as a Visual Task," p. 110) that "it was possible to eliminate certain subjects from the group whose responses were found to be erratic." Unless a definite cause of irregularity in the working of any technique can be found it is surely not justifiable to neglect some of the data in this way.

Dr. Luckiesh refers to "the risk of disturbance of a mechanical counter." This counter attached to the eyelid was used only in those experiments performed in the dark, when the blinks could not be seen, and also in some of the long experiments. In all others the blinks were counted visually by the experimenter.

I.E.S. Fellowship

The following application for Fellowship has been accepted:—

Nelson, Dr. J. H.

The Illuminating Engineering Society is not as a body, responsible for the opinions expressed by individual authors or speakers.

With a view to avoiding possible confusion with other publications, reference to these Transactions should be in the form:—"Trans. Illum. Eng. Soc. (London)."

THE ANNUAL GENERAL MEETING

The annual meeting of the Illuminating Engineering Society took place in the Lecture Theatre in the Institution of Mechanical Engineers, at 4.30 p.m., on Tuesday, May 9, 1944, when the President (Dr. H. Buckley) was in the chair.

The Hon. Secretary having read the notice convening the meeting and also the statement of the auditors in regard to the accounts for the past year, it was agreed that the minutes of the last meeting, which had been published in THE TRANSACTIONS, should be taken as read.

The President then presented the Report of the Council and statement of accounts for 1944.

Adoption of Report and Accounts

The following resolution was then put to the meeting by Mr. E. W. Murray and seconded by Mr. M. W. Hine, both of whom expressed appreciation of the evidence of prosperity contained in the Report:—

That the Report of the Council and the accounts of the Illuminating Engineering Society for the period January 1, 1943, to December 31, 1943, be hereby adopted, and that a vote of thanks be extended to the President, Council, and officers for their efforts on behalf of the Society during the past session.

On being put to the meeting the Report was declared carried unanimously.

The President, on behalf of the Officers and Council, expressed his appreciation of the vote of thanks and added that the satisfactory results achieved last year had been due not only to their efforts but also to work done by a considerable number of members in London and the Provinces.

Election of Officers and Council

The President then proceeded to refer to events that had taken place since the period covered by the Report.

In regard to elections for the forthcoming session he stated that no independent nominations for officers had been received. Therefore the Council's nominations stood, and those elected were as follows:—

President: Mr. E. Stroud.

Vice-Presidents: Mr. H. C. Weston, Mr. Dean Chandler, and Mr. Howard Long.

Honorary Treasurer: Mr. N. V. Everton.

Honorary Secretary: Mr. J. S. Dow.

The report of the scrutineers of the ballot for members of the Council was then presented, and it was announced that the following had been elected:—

Mr. P. V. Burnett, Mr. W. J. G. Davey, Mr. W. Hetherington, Mr. R. Maxted, Mr. C. W. M. Phillips, Mr. George Smith, Mr. J. M. Waldram, and Mr. G. T. Winch.

On the motion of the President, the scrutineers were thanked for their services.

Future Developments

Referring to progress during the present year, the President recalled that two new groups, in Huddersfield and in Teesside, had been formed. He also alluded to the work being done in connection with lighting reconstruction and the issue of three pamphlets dealing with "The Principles of Good Lighting," "The Lighting of Public Buildings," and "The Lighting of Schools." The Society had carried out, at the request of the Ministry of Transport, an investigation into the brightness and legibility of traffic signs. The experiments had been conducted in the Research Laboratories of the General Electric Company, Ltd., whose assistance had been most valuable, and the Society had been warmly thanked by the Ministry for their efforts.

In conclusion, the President referred to proposals in regard to the post-war activities of the Society, which had been explored by two committees in relation to replies to the questionnaire issued by Mr. Ackerley during his period of office as President. The realisation of these aims, however, would involve reorganisation of the administrative staff at headquarters and the appointment of a full-time paid secretary, and there were various important questions, including finance, that would require careful consideration.

It was then proposed by Mr. J. S.

Smyth and seconded by Mr. G. Herbert:—

> That the meeting records the appreciation of the services of Messrs. Robert J. Ward and Co. as auditors of the Society and approves their re-election for the next session.

This resolution, too, was declared carried unanimously. This terminated the formal business.

Address on "Tolerances and Their Effect on Physical Measurements"

Following the annual general meeting a sessional meeting took place. After the minutes of the last meeting had been taken as read the President called upon Sir Charles Darwin, Director of the National Physical Laboratory, to deliver his address on "Tolerances and their Effect on Physical Measurements." Sir Charles remarked that in the past tolerances such as arise in the manufacturing processes had been regarded as a nuisance. Recent developments in theoretical physics had, however, shown that this subject of errors and tolerances was not only interesting in itself but important in the foundation of physics. The effect of tolerances was then illustrated by a number of simple examples, such as the estimation of the number of an audience present in a room, the performance of a machine used for weighing bullets, and the recognition of the correctness of a musical note. Turning next to optics, the lecturer explored the idea of resolving power of microscopes and the "Uncertainty Principle," which was, perhaps, only one of a number of limitations to tolerances in nature. The address was concluded by examples from pure mathematics, and it was pointed out that irrational numbers might have a definite significance provided a certain tolerance was agreed. [The full text of this address appears on pp. 119-125 in this issue.]

The vote of thanks to the lecturer was moved by Mr. Percy Good, who said that they had all been intrigued not only by the subject-matter of the lecture but by the manner in which it had been delivered. He himself had been greatly interested in this subject, which had considerable application in industry and was of special interest to the Illuminating Engineering Society. In the case of lamp manufacture inaccuracies had led to a system of control resulting in a high value of excellence which had been retained during the war period. The Society was interested in the development of a standard eye and perhaps others had established a standard ear. Nobody, apparently had endeavured to establish a standard man or suggested that tolerances should be based on his frailties. It might, however, be of interest to recall a statement, he believed attributed to Plato, that "All civilised order survives on its merits and is transformed by its power of recognising its imperfections." To have had a picture which represented science as an imperfection was a great achievement; it was interesting to note that there were cases in which it was necessary, for the perfect working of an appliance, to introduce some form of imperfection.

Mr. J. G. Holmes, in seconding the vote of thanks, said that they welcomed Sir Charles not only as the bearer of a famous name, but as the director of the National Physical Laboratory, to which illuminating engineers were greatly indebted. There was a time, when science was just emerging from alchemy, when the scientist thought he could regard himself as being always right. We now realised the limitations to our knowledge and the imperfections of our instruments—that was a great advance in knowledge. They were grateful for this clear statement on the importance of errors and tolerances, which played a great part in photometry. Sir Charles had also illustrated the importance of statistics, and he welcomed this demonstration because it was often necessary in connection with illumination to use statistics in order to interpret results and get an answer of known precision. He had known Sir Charles as a physicist and mathematician and more recently as an administrator. During the last hour he had come to know him as a clear and sympathetic teacher, and it had been a great joy to follow him into the realm of tolerances.